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(2)

SECURITY CLASSIFICATION OF THIS PAGE

REPORT DOCUMENTATION PAGE

Form Approved
OMB No. 0704-0188

1a. REPORT SECURITY CLASSIFICATION: UNCLASSIFIED

1b. RESTRICTIVE MARKINGS

2a. SECURITY CLASSIFICATION AUTHORITY

AD-A216 417

3. DISTRIBUTION/AVAILABILITY OF REPORT

5. MONITORING ORGANIZATION REPORT NUMBER(S)
AFOSR-TR- 89-1777

DTIC ELECTE JAN 04 1990 S E D

6a. NAME OF PERFORMING ORGANIZATION: Duke University

6b. OFFICE SYMBOL (If applicable)

7a. NAME OF MONITORING ORGANIZATION: AFOSR/NL

6c. ADDRESS (City, State, and ZIP Code)
Department of Psychology
Durham, NC 27706

7b. ADDRESS (City, State, and ZIP Code)
Building 410
Bolling AFB, DC 20332

8a. NAME OF FUNDING/SPONSORING ORGANIZATION: AFOSR

8b. OFFICE SYMBOL (If applicable): NL

9. PROCUREMENT INSTRUMENT IDENTIFICATION NUMBER: AFOSR-87-0353

8c. ADDRESS (City, State, and ZIP Code)
Building 410
NBolling AFB, DC 20332

10. SOURCE OF FUNDING NUMBERS

PROGRAM ELEMENT NO.	PROJECT NO.	TASK NO	WORK UNIT ACCESSION NO.
61102F	2313	A6	

11. TITLE (Include Security Classification)
Categorizing Sounds

12. PERSONAL AUTHOR(S)
Gregory R Lochead

13a. TYPE OF REPORT: ANNUAL

13b. TIME COVERED: FROM 1 Oct 88 TO 31 Sep 89

14. DATE OF REPORT (Year, Month, Day): 1 Dec 89

15. PAGE COUNT

16. SUPPLEMENTARY NOTATION

17. COSATI CODES

FIELD	GROUP	SUB-GROUP

18. SUBJECT TERMS (Continue on reverse if necessary and identify by block number)

19. ABSTRACT (Continue on reverse if necessary and identify by block number)

Results of the past year are consistent with a model having two assumptions: Successive sounds are remembered as overly similar, and subjects attempt to correct for this by adjusting their response scales. This holds for unidimensional and multidimensional stimuli. In addition, when two dimensions are varied (loudness and pitch were examined here) but only one is judged, trial-to-trial variations of the other dimension interfere with performance. The magnitude of this interference is greater when the other, irrelevant dimension varies by larger amounts.

20. DISTRIBUTION/AVAILABILITY OF ABSTRACT
☒ UNCLASSIFIED/UNLIMITED ☐ SAME AS RPT. ☐ DTIC USERS

21. ABSTRACT SECURITY CLASSIFICATION

22a. NAME OF RESPONSIBLE INDIVIDUAL: JOHN F TANGNEY

22b. TELEPHONE (Include Area Code): (202) 767-5021

22c. OFFICE SYMBOL: NL

DD Form 1473, JUN 86

Previous editions are obsolete.

SECURITY CLASSIFICATION OF THIS PAGE
UNCLASSIFIED

90 01 04 037

ON CATEGORIZING SOUNDS

Gregory R. Lockhead

Department of Psychology

Duke University

Durham, North Carolina 27706

DGREG@TUCC.BITNET

AFOSR-TR- 89-1777

Grant No. AFOSR-87-0353

Interim Report

Period covered: 1 October 1988 to 30 September 1989

Grant Monitor: John F. Tangney, Program Manager,

Life Sciences Directorate

Prepared for: Directorate of Life Sciences

Air Force Office of Scientific Research

Bolling Air Force Base

Washington, D. C. 20332-6448

Abstract

Context is important when people judge sounds or their attributes. Judgments depend on 1] what sounds recently occurred (sequence effects), 2] how the various sounds in the study differ from one another (range effects), 3] the distribution of those differences (set effects), 4] what the subjects are told about the situation (task effects), and 5] what the subjects are told about their performance (feedback effects). These effects determine the overall mean and variability of both response times and response choices, and they determine performance on individual trials. These results are consistent with a model having two assumptions: Successive sounds are remembered as overly similar, and subjects attempt to correct for this by adjusting their response scales. This holds for unidimensional and multidimensional stimuli. In addition, when two dimensions are varied (loudness and pitch were examined here) but only one is judged, trial-to-trial variations of the other dimension interfere with performance. The magnitude of this interference is greater when the other, irrelevant dimension varies by larger amounts. These data support the suggestion made here that continuing to search for underlying psychophysical scales may not be productive. That traditional approach uses methods adopted from classical physics to study how attributes of objects are processed. In its place, it is considered that a biological approach which considers how organisms perceive objects in environments might better describe how sounds and other stimuli are judged.

ON CATEGORIZING SOUNDS

(a,b) Objectives and status of the research effort.

The general goal of this project is to better understand how complex sounds are identified. The specific goals being supported by AFOSR are to evaluate a proposed model of sequence effects in univariate tasks and to learn if that model generalizes to multi-dimensional judgments. Recent progress on those goals is summarized in this report. In addition, a new goal to restate psychophysical models in the language of biology rather than that of physics is suggested.

This work is based on the well documented fact that what a sound or any other stimulus is identified to be is not determined just by that stimulus. Judgments depend importantly on many factors. Using sounds for the stimuli, this project has focused on five features of this fact. Judgments depend on 1] the physical differences between stimuli (range effects), 2] the temporal order in which those stimuli occur (sequence effects), 3] for multidimensionally varying stimuli, how stimulus attributes are combined to produce complex sounds (set effects), 4] what information is given to the subjects about the situation (task effects), and 5] what information subjects are given about their performance (feedback effects).

These effects - range, sequence, set, task, feedback - are important for many reasons. Practically, they must be taken into account if we are to accurately predict performance in a given situation: Range has modified responses by a factor of six,

sequence and task have each affected choices by 75% of the response range, and set has shifted identification accuracy from near chance to near perfect (Lockhead, 1984). Theoretically, factors that modify behavior by such large amounts need to be understood. Scaling models cannot be evaluated unless they are.

Context effects in univariate data.

Three dependent variables have been used to measure context effects when univariate stimuli are judged. These are average responses or choices, response variability, and response times. Findings summarized previously (Lockhead, 1988b) include: Response variability is larger in stimulus sets having a larger stimulus range, response choices are shifted further on trials that successive stimuli are physically more different, and assimilation is greater in conditions of greater stimulus range. These results allowed the conclusion that context effects described in the literature as due to stimulus range (a between-conditions measure) are, instead, due to differences between successive stimuli (a within-conditions sequence effect).

Context effects in bivariate data.

Examinations of context effects in multidimensional stimuli introduced earlier (Lockhead, 1988b) is extended here. Using ten tones sawtooth correlated (cf. Lockhead, 1972) in loudness (79 to 88 dB SPL in 1 dB steps) and pitch (1000 to 1045 Hz in 5 Hz steps), people were asked to identify the intensity of each tone when feedback (the numerals 1 - 10 corresponding to the intensity

level) was given and, in other conditions, when feedback was not given after each response.

<u>When feedback was given</u>, response choices to the sawtooth paired stimuli tended toward the value of the prior stimulus intensity and toward the value of the prior stimulus frequency. Judgments often depended on the previous stimulus by one-third of the response range.

Response times also depended on sequence. Responses were faster when successive stimuli were physically more similar [similarity was measured as the Euclidian distance between stimuli in the frequency-amplitude space] ($r = 0.87$, $p < 0.01$).

<u>When feedback was not given</u>, there was again assimilation between the response and the prior stimulus, and assimilation between the response and the prior response in this sawtooth paired paradigm. Also again, response times correlated with the difference between successive stimuli ($r = 0.68$, $p < 0.01$).

Thus, there are systematic and stable sequence effects in bivariate data. Analogous to univariate cases, responses assimilate toward the prior stimulus or response, and when successive stimuli are more different response times are longer. These results are for when loudness was judged.

I have replicated this study except with different subjects and with pitch judged rather than loudness. Again, no individual differences were noted except that some subjects were more consistent than others. Also again, there were marked sequence effects on choices and on response times. Choices are biased toward the values of the prior response and prior stimulus

(assimilation) and response times are longer when successive stimuli are differ more.

Except that the magnitudes of these sequence effects when pitch was judged are smaller than those when loudness was judged by different subjects, no new findings were noted in this study.

Orthogonal sorting.

In the above studies, amplitude and frequency were correlated. Here, continuing work introduced last year (Lockhead, 1988b), auditory amplitudes were paired orthogonally with auditory frequencies. People were asked to classify each tone according only to its loudness, i.e., ignore its pitch, or to classify each tone according only to its pitch, i.e., ignore its loudness.

Method and Procedure. In different conditions, subjects judged loudness [pitch] when pitch [loudness]: (1) did not change between trials, (2) could differ a small amount between trials, and (3) could differ a large amount between trials.

When loudness was judged the same intensities were used in all conditions, 79 and 81 dB. There were three orthogonal conditions, called narrow, intermediate, and wide. The two amplitudes were presented randomly at 1000 and 1015 Hz (narrow range), or at 1000 and 1045 Hz (intermediate range), or at 1000 and 1500 Hz (wide range). In control (univariate) conditions, the amplitudes were presented at 1000 Hz. or at 1500 Hz.

Analogously, when pitch was judged those frequencies (1000 and 1015 Hz) and were randomly presented at amplitudes of 62 and 68 dB (narrow range), 68 and 80 dB (intermediate range), or 62

and 80 dB (wide range). In control conditions, the frequencies were presented consistently at 62 dB or at 80 dB.

Six subjects gave 400 responses in each condition. They classified each tone as quiet or loud, or as low or high pitch, by pressing the left or right of two buttons.

Results and Discussion. For every comparison available, performance was significantly faster ($p < 0.01$) in the univariate tasks than in the orthogonal tasks. When loudness was judged orthogonal responses were, compared to the relevant univariate condition, slower in the narrow and intermediate range conditions (frequency varied from trial to trial by 15 or by 45 Hz) and even slower in the wide range condition (frequency varied by 500 Hz).

When pitch was judged responses were faster in general to 80 dB tones than to 68 or 62 dB tones. Compared to the relevant univariate controls, orthogonal responses were slower when intensity varied by 12 or 18 dB, and there was a numerical but not a reliable difference between the control and the narrow range condition when intensity varied by 8 dB.

In all instances, error rates correlated positively with response times.

This observation that responses are slower in orthogonal conditions than in univariate conditions replicates earlier reports (e.g., Garner, 1974). The further observation here is that the magnitude of this effect is larger when the stimuli differ by more on the irrelevant dimension. This finding is consistent with a general thesis of this project; performance is

more variable when successive stimuli are more different from one another.

To examine if this range effect (slower responses when the stimulus range is larger) is due to sequence rather than range per se, sequential response times were evaluated within each condition. Responses were fastest on trials that the irrelevant stimulus level repeated, and were progressively slower when successive stimuli were more different on the irrelevant dimension. This is the case, separately, when the relevant stimulus level did not change between trials (response repetition) and when the relevant stimulus level did change between trials (response change) [all $ps < 0.01$].

Discussion. Trial-to-trial variation in the irrelevant dimension (pitch or loudness) affects response times to the relevant dimension (loudness or pitch) in these bivariate tasks. Judgments take more time, are more variable, and have more errors when the irrelevant dimension varies between trials than when the irrelevant dimension is held constant from trial to trial. Furthermore, the amount of this interference is greater when the magnitude of this stimulus change is greater.

One factor muddies this result from being a crisp statement. This is that irrelevant variation in auditory frequency affects loudness judgments more than irrelevant variation in auditory amplitude affects pitch judgments. This interaction is not addressed here but will be discussed in later work.

Assimilation in memory.

There is assimilation between successive responses. The clearest such demonstration is that assimilation occurs in guessing studies where there are no stimuli (cf. Lockhead, 1984). This does not mean, however, that assimilation does not also occur in perception or in memory.

The possibility that successive perceptions or memories do assimilate is suggested by data reported in Lockhead and King (1983). They asked subjects to report the relative intensities of successive tones. These were 30 sinewaves separated by 1 dB steps and presented randomly. Consider when the same tone, say 74 dB, repeated on successive trials. That ratio is $S_N/S_{N-1} = 74/74 = 1$ and so the response should also be 1. This rarely happened. Instead, the response tended to be greater than 1 if the tone before these two (S_{N-2}) was less than 74 dB, and the response tended to be less than 1 if S_{N-2} was greater than 74 dB. Apparently, the first 74 dB tone (S_{N-1}) assimilated in memory toward S_{N-2} (or was perceived as overly like that earlier tone - there is no way here to distinguish between assimilation in memory and assimilation in perception), and S_N was compared to that biased memory. Thus, the judged ratio was large when S_{N-2} was small and small when S_{N-2} was large.

To pursue this suggestion that sequence effects occur in memory or perception, as well as in responses, I had four people identify both the loudness and pitch of each of ten tones. These stimuli were the ten sawtooth paired loudnesses and pitches used in the identification study summarized above. Subjects in that earlier study knew (were told) there were ten tones. Here, the

subjects were told nothing about the structure of the stimulus set. They were simply asked on each trial to categorize each loudness with the numbers 1 - 10 and, separately on the same trial, to categorize each pitch with the numbers 1 - 10. Thus, no explicit identification function was given the subjects.

Various outcomes might be predicted. If subjects learn the stimuli during the experiment, then they might come to realize that only 10 different tones are involved. However, if there is assimilation between successive perceptions, then the subjects might come to hear all of the stimuli as more and more alike (since each sounds overly like the previous one) and thus come to conclude there are very few different tones. Or, if there is assimilation between successive memories (or between perception and memory) then many different memories would be built up; there might be a different memory for each stimulus depending on what stimulus preceded it, or memories of 100 tones.

These three considerations suggest three different outcomes. Subjects should come to behave as if there are 10 tones (learning), or few tones (assimilation in perception), or many tones (assimilation in memory).

Results. When these 10 stimuli were judged in the sawtooth paired condition reported earlier accuracy was about 50% correct. Those subjects were told the stimulus set was composed of 10 loudnesses that were perfectly but nonlinearly correlated with 10 pitches.

In the current study, no information as to the number of different tones was given the subjects. According to the

subjects' reports at the end of the study, the correlation between pitch and loudness was not detected. In the data, each subject used 90 or more of the possible 100 responses (each of the 10 loudness responses X each of the 10 pitch responses).

The dependent variable of primary interest here is not accuracy or response times. Instead, it is each subjects' estimate as to how many different tones had been presented during the study. They were asked this, unexpectedly, after 400 trials. These estimates were 68, 80, 98 and 100 tones. Following an additional 400 trials on the following day, these subjects were again asked to estimate how many different tones had been presented. These estimates were 37, 50, 75, and 90 tones. Of course, only ten different tones had been presented during the 800 trials.

While this does not constitute a proof, the result is consistent with the inference that assimilation occurs between memories of successive tones [as well as between successive responses]. This interpretation is consistent with the memory model described in the initial proposal of this research. According to that model, the response to a stimulus, R_N, assimilates toward the value of the memory of stimulus on the just previous trial and contrasts from memories of earlier trials:

$$R_N = S_N + a(M_{N-1} - S_N) + b(\bar{M} - M_P) \qquad (1)$$

where S_N is the stimulus, M_{N-1} the memory of the previous stimulus, $\bar{M}$ is the average memory of all stimuli during the experiment, M_P is the average memory of stimuli on trials N-2 to

N-7 and called the memory pool, and a and b are positive constants.

Conclusion. Context effects demonstrate that numerical judgments in any particular psychophysical task cannot be predicted precisely by a general psychophysical equation (i.e, one not constructed for that situation). For example, the slope of the power function calculated from magnitude estimations of the loudnesses of 1,000 Hz sinewaves can depend by a factor of three on the stimulus range (Lockhead & King, 1983). Furthermore, the judgment of a stimulus on any particular trial within a task also cannot be predicted precisely by an equation based only on the stimulus intensities in that task (i.e., an equation that does not take sequence or momentary context effects into consideration). Indeed, performance is sometimes better predicted by knowledge of only the prior response than it is by knowledge of only the current stimulus (Lockhead, 1984). Thus, while some traditional psychophysical scaling model might describe summary performance in a particular condition, it cannot describe performance on individual trials within that condition and it does not regularly generalize to different conditions.

Comment. The fact that there are context effects in psychophysical data does not necessarily mean there is no underlying psychophysical scale. An example from physics is appropriate. If one is attempting to measure the rate at which objects fall, a blowing wind makes it difficult to evaluate the gravitational constant, g. Nonetheless, the constant is real.

Similarly, context effects could simply make it difficult to measure the psychophysical scale, which might also be real. Indeed, for purposes of psychophysical theory Luce and Krumhansl note that effects of sequence are "often viewed as a mere nuisance" (1988, p. 52) by researchers. Ostensibly, this is because those effects interfere with the search for the underlying psychophysical scale, much as air interferes with measuring *g*. According to such a view, it becomes essential to control or otherwise measure context effects so the noise associated with them can be removed. That would allow better evaluation of the sought scale.

But the situation is not this simple and Luce & Krumhansl are themselves not as sanguine as the above quote may suggest. They closed their chapter in the 1988 edition of *Stevens' Handbook of Experimental Psychology* with the observation that "One cannot but be concerned by the demonstration (King & Lockhead, 1981) that the exponents [of psychophysical scaling functions] can easily be shifted by as much as a factor of 3 ... Clearly, much more work, using the data from individual subjects, is needed before we will be able to develop any clear picture of the structure of psychophysical scales." (p. 67)

While more work surely needs to be done, I know no evidence to suggest that new insights might come from studying individual subjects, although they might. Rather, the view I plan to pursue is that the reason psychophysical scaling theory is difficult to demonstrate is not because context effects make testing difficult. Instead, the context effects discussed here and others

reported by Marks (1989) plus the constancies and simultaneous contrast all cast doubt on the validity of <u>any</u> model of an underlying psychophysical scale. Except for its esthetic appeal there seems to be little reason to expect a fixed relation between behavior and the amount of energy in some attribute of a stimulus.

The alternative suggestion made here is that a psychophysical approach based in biology might be more productive than one grounded in physics. Traditional psychophysical scaling models reflect classical theory in physics. For example, the volume and temperature of a gas are linearly related both phenomenally and algebraically. Increasing the temperature increases the volume. Similarly, acoustic energy and loudness are linearly related both phenomenally and algebraically. Increasing the amplitude increases the loudness. This parallel between phenomenology and algebra was useful in classical physics and gave credence in psychology to a psychophysical model of the same form.

Such equations work well, within limits. However, this does not necessarily mean that underlying or causative factors are captured by those equations. In fact, the classic physical model to describe the action of gases was determined to be wrong and was replaced with a thermodynamic explanation. That is, a theory that is based on underlying properties replaced the theory based on directly observable properties.

Eventually, theories of psychological scaling that are based on observable properties might also be replaced by theories based

on underlying properties. This at least seems necessary for a theory to relate luminous energy and brightness. Rather than Fechner's Law or Stevens' Law or another law stated in terms of energy, a more correct model would be stated in terms of changes in luminous energy over time. One reason is that nothing is seen if energy at the eye does not change over time (probably within 200 msec, Lockhead, 1988a). Perhaps the most dramatic demonstration is that stabilized retinal images quickly disappear. Without temporal luminance transients, we are blind.

In addition to time, any complete model of brightness or of a brightness scale must also include effects of simultaneous contrast and of remote contours. The Craik-O'Brien-Cornsweet effect clearly demonstrates that the amount of energy at a site does not predict brightness at that site. In fact, it does not even predict brightness relations. However, luminance changes at remote contours do predict brightness relations (Arend et. al, 1971).

No current psychophysical models provide for such facts, except they do allow there to be different constants in the scaling equations for different stimulus particulars. The difficulty then is, because there are so many particulars and because there is no way to predict the effect of any of them within the psychophysical scaling theory, that the theory becomes reduced to a set of empirical statements with little generality.

Many of these noted difficulties and others have been known for a long time. However, unlike in physics, scaling models have not given way to some more fundamental view. One reason may be

that the level of many psychophysical models can be valuable. For example, a bril scale is convenient for rating light fixtures. Another reason may be that the need for a psychophysical function other than one that correlates with phenomenology is not compelling.

Another reason scaling models have not been replaced is that the situation is simply difficult. Certainly I am not able to offer a complete psychophysical theory based on underlying properties. Nonetheless, there is enough evidence to support a search for a theory at some level other than classical physics or phenomenology. The following skeleton describes a beginning for such a search. The suggestion is based in biology and in the theory of evolution. This is because psychophysics is concerned with reactions of biological organisms, and because I take as a premise that evolution of the ability by organisms to perceive objects in places is more fundamental than evolution of their ability to abstract and measure intensities of attributes.

From an evolutionary perspective, it is difficult to argue that coding the intensity of attribute of an object, such as its brightness or its loudness, is essential. Such information is not fundamental to the task of identifying the object. Furthermore, as discussed ahead, knowing the absolute intensity of attributes of the object can be interfering. This is because energies may change with the environment while the object itself does not change.

A biological view is not inconsistent with most psychophysical writings. However, it is also not commonly consid-

ered in that literature. A brief example that the common view is based in classical physics is seen in recent attempts by Lester Krueger and 31 sets of commentators to reconcile Fechner and Stevens (Behavioral and Brain Sciences, 12, June, 1989). Although those 32 papers note some difficulties with one or another theory, or the inconvenience of some particular data, or that some model does not satisfy a Popperian or other philosophical observations, or how discouraging the search has been, at least 30 of them are based in classical physics.

Of the possibly only two excepting comments, one (Brysbaert and d'Ydewalle) shows that the difference limen for brightness depends nonmonotonically on the background on which the stimulus is viewed. This simultaneous contrast effect has been known for a very long time. It has largely been ignored in psychophysics, apparently because it is seen as an inconvenient perturbation [perhaps like sequence effects] to only be addressed after the more fundamental psychophysical scale has been determined. The second exception (Shepard) proposes a psychophysical law based on features of the world in which organisms evolved rather than one based on the organism directly.

The remaining 30 views are consistent with a classic approach. Fechner's insight continues to be important. Even so, I believe it might be productive to modify Fechner's approach by considering psychophysical scaling data in terms of the task and environment in which stimuli are presented to a biological organism, rather than in terms of an abstracted attribute of an isolated stimulus. In retrospect (I have worked a long time

within classical psychophysics) it is amazing that biology has not been a basic interest in psychophysics. But it has not. No major biologist is referenced in any of the 32 Behavioral and Brain Sciences papers (1989, references on pp. 312-320) or in the core paper on Measurement, Scaling, and Psychophysics in Stevens Handbook... (1988, references on pp. 67-74).

From a biological perspective, there are many reasons to not expect a psychophysical function of intensities or of attributes. One reason follows from the fact that, in the real world, intensities from the same object differ in different environments. Since identifying objects and object locations is fundamental to organisms, knowing such unreliable values is not productive for survival. Whether a tiger is seen in the shadow or in bright light, it must be perceived as a tiger. In part, brightness constancy provides for this. Knowing how much light is coming from the fur does not.

Stevens was obviously aware of something like this because he found it necessary to state that people have "the ability to separate out of a complex configuration one single aspect and to compare that aspect with the same aspect abstracted from another configuration" (1975, p. 66). Perhaps similarly, I (Lockhead, 1972) and many others have suggested that after an object is perceived it may be analyzed into attributes which might be judged.

But these judgments are often not veridical. They depend on the stimulus in which they are embedded, on its background, and on other possible alternatives in the situation. Brysbasert and

d'Ydewalle show this for a brightness and its surround, I have shown this for extents and locations and other dimensions (Lockhead, 1966, 1972, 1984, 1988b), and many others have shown this for many different stimuli. The literature on interference in orthogonal [filtering] sorting tasks provides hundreds of demonstrations that people cannot veridically abstract values of an attribute from an integral object (cf. Garner, 1974; Pomerantz, 1989).

The assumption for psychophysics that an attribute can be judged independent of its environment is simply wrong. One can obtain a regular psychophysical function for averaged data based on values of some attribute in a fixed environment of otherwise unchanging stimuli. However, that function is commonly different for the same intensities when they are scaled in different environments or when they are features of different objects.

Some researchers might consider that the reason the psychophysical scale changes with situations is because the "true" scale is somehow perturbed. While this might be the case, it appears impossible to demonstrate. Furthermore, the ever present trial-by-trial effects within each condition mean that this "true" scale then depends on the particular trial, as well as on the particular condition. The scale changes within conditions and between conditions and is a will-o'-the-wisp.

Conclusion. Historically, there have been two important classes of psychophysical study. These have been called Class A and Class B, or sensory and perceptual, or local and global, and probably

other terms. Among Class A studies, detection, two-choice discriminability and other local measures reveal differential sensitivities of receptor or sensory systems. Among Class B studies, absolute identification, magnitude estimation, cross modality matching, and other global measures reveal aspects of feature or object judgment systems.

By this distinction, local measures provide limits for global measures. However, neither provides the necessary information for predicting what amount or quality a subject will assign a particular stimulus or attribute of that stimulus, either on average over the course of the study or on a particular trial. The interpretation here is this is because the observer's initial analysis is not of an object or an attribute in isolation. Rather, it is of objects or attributes in relation to an environment. There are many demonstrations. In an Ames distorting room, the dog appears large or small depending not on the dog but where it is in the room. In a magnitude estimation experiment, a sound is judged loud or not loud depending on what sounds preceded it. In a brightness matching task, identical patches appear identical or very different, depending on what surrounds each of them. Simply, we are unable to process attributes independent of the environment. Perhaps, analogous to the deconstruction movement in the humanities which concludes that the meaning of any text is determined by its context, stimulus judgments are determined by the environment.

To date, the major accomplishment of this research effort has been realizing and partially documenting the necessity to

study the perceptions and memories of relations between items and surrounds in order to understand how objects and attributes are judged. By this view, rather than being a mere nuisance the study of context is fundamental to understanding how organisms perceive objects and attributes.

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(d) Personnel supported by AFOSR.

Gregory Lockhead, PI, fully supported Summers 1988 and 1989.

Ronald Collis, Graduate student. Partially supported academic years 1987-8 and 1988-9; fully supported Summers 1988 and

1989. Collis left Duke in September, 1989.

Michael Hunseder, undergraduate student. Supported 5-10 hours per week, Spring, 1989.

Michael Hunseder and Matthew Rohdie, undergraduate students. Each currently supported 5-10 hour per week.

(e) Interactions.

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(f) Inventions, etc.

none

(g) Statements.

none